Ocean Sci. Discuss., 9, C15–C19, 2012
www.ocean-sci-discuss.net/9/C15/2012/

Ocean Science Discussions

OSD

9, C15–C19, 2012

Interactive Comment

Interactive comment on "Interannual coherent variability of SSTA and SSHA in the Tropical Indian Ocean" *by* J. Q. Feng

Anonymous Referee #2

Received and published: 13 February 2012

The present paper analyses the covariance of the SST and SSH fields in the tropical Indian Ocean with satellite and in situ measurements. On this basis the author expects to determine the influence of the subsurface variability on the SST. For the SSH fields he uses a multi-satellite product and for the SST fields he uses the NCEP reanalysis based on both in situ and satellite measurements. The study is made up of two parts. In the first part, the author exhibits and describes the SST-SSH co-variability in the Indian ocean with a Singular Value Decomposition of the cross-covariance matrix of both variables. In the second part, he intends to unfold the mechanisms that underpin this SST-SSH co-variability by the use of the Extended Associate Pattern Analysis method. The author concludes that 1) the 2 dominant modes of the SSH-SST co-variability in the Indian Ocean have a main period of 3-5 years, 2) that the first mode is related to ENSO and the second one is the evolution of the first mode after 10 month 3) that the

SSH-SST co-variability can be mainly explained by the propagation of oceanic waves (Rossby and Kelvin). In the first part of the paper, the author gives a quite satisfactory estimation (even though a few technical points still have to be clarified, see remarks below) of the dominant modes of the SSH-SST co-variability in the Indian Ocean. This estimation is consistent with the literature (Leuliette et al. 1999). The patterns shown in Fig. 1, closely resemble the patterns shown by Leuliette et al. (1999) in their Fig. 5 (in the Indian ocean) , while the patterns shown in Fig. 2 are very close to the opposite of the patterns shown by Leuliette et al. (1999) in their Fig. 6. The author finds a correlation between the first SVD mode expansion coefficient with an ENSO proxy (Nino 3.4) as Leuliette et al. (1999) already did at global scale. In this sense I think that this first part is not very original: it is indeed a reliable estimation of the dominant modes of the SSH-SST co-variability in the Indian ocean but it does not bring substantially more information than what was already published by Leuliette et al. (1999) at global scale (even though it is almost up to date). In the second part, the author explains that the second SVD mode lags the first one by 10 month and that both modes are mainly explained by propagation of Rossby and Kelvin waves. While I am quite convinced by the EAPA analysis that the second SVD mode (or most of it) lags the first SVD modes by about 10 month, I am not convinced at all by the demonstration that the propagation of oceanic waves mainly explains the SSH-SST co-variability. The author claims that both SVD modes present features of large oceanic waves but this assertion is very weakly supported. There is no justification of this at the scale of the basin. Only in the region 13°S to 9° S the longitude-time plots given in fig. 6 presents indeed typical features of Rossby waves (but there is no check of the phase speed and comparison with already published values see Chambers et al. 2000, Rao and Behera 2005). This very local result is centred on a very specific region of the Indian ocean: the thermocline ridge. In this region the thermocline is very close to the surface so we can expect Rossby waves to make SSH and SST co-variate indeed. But this local phenomenon, largely known and documented (see Schott et al. 2009), can not be considered representative of the whole Indian basin as the author seems to suggest. On no account it can justify the conclusion of the author that at basin scale the SSH-SST co-variability can be mainly explained by the propagation of oceanic waves. In conclusion, I think that this article gives a satisfactory almost up to date estimation of the dominant modes of the SSH-SST co-variability in the Indian Ocean but it fails in showing that Rossby and Kelvin waves explain most of this co-variability. Unfortunately, already satisfactory estimations of the dominant modes of the SSH-SST co-variability have been published in Leuliette et al. (1999). Moreover a recent study by Rao and Behera (2005), not even referenced here, have already analysed in details the influence of subsurface on the SST and the role played by Rossby waves in both SSH and SST variability. The paper of Rao and Behera (2005) goes far beyond the results achieved here. Consequently I think that the present article does not provide significant improvements to already published works and I recommend to reject the paper. An interesting originality of the present paper is that it is based only on measurements. The author should read Rao and Behera (2005). On this basis he could give his own estimation of the subsurface variability influence on SST based on measurements only (he could even analyse extra fields such as heat content and wind). It would be a very interesting study and more valuable than the present work if any differences arise with the SODA based study of Rao and Behera (2005) for example.

Other major comments

Page 6 line 5 to 12: the introduction of the EAPA method is too short and should be explained with more details.

Page 6 line 20: I am not totally convinced by the SVD analysis performed in this paper. The author has to show that the SVD modes extracted from the co-variability of the SSH-SST fields represent a significant part of the total variance of each field (the SST field and the SSH field). If the SVD modes represent only a few percentages of the total variance of the SST field for example (or the SSH field) this would mean that he is looking at insignificant signal in terms of SST (or SSH). Nevertheless, given the study of Leuliete et al. (1999) I guess that the signal is significant both in terms of SSH and

SST but it has to be proved properly. Another way to do so is to compute the temporal coefficient "rtime" as in Leuliette et al. (1999). Whatever the method used, the author has to add such a kind of test to convince readers of the significance of his SVD modes both in terms of SST and SSH.

Page 7 line 27: the author assumes that the first and second SVD modes are related to ENSO events because the time series show maxima in 94-95, 97-98,02-03, 06-07. But the IOD also shows maxima at these dates. In particular 94-95 is a strong maximum in the IOD proxies while it is weak in ENSO proxies. The relation of SVD modes with the IOD or ENSO is not clear at all. The author assumes that it is linked to ENSO because he has not looked at the IOD but other authors (Rao and Behera 2005 for example) see a stronger influence of the IOD. The author should discuss the role of the IOD and intend to see what is linked to IOD, to ENSO and to both.

Page 8 line 26: to convince readers that Rossby waves can be seen on Fig. 6 the author has to evaluate the phase speed and compare it to both the theoretical value expected at this latitude and the value found in the literature already.

Figure 1 and 2: there are no units on these figures!

Figure 3: how is computed the 95% confidence level? It should be stated either in the caption or in the text

Minor comments

Page 3 line 14: I do not think SSH can influence SST. The author probably means that SSH is a proxy of the subsurface variability so we can track in the SSH fields some changes in the subsurface variability that influence both SSH and SST. This sentence should be rephrased.

Page 5 line 3: Where did the author get the SSH dataset from? Is it the AVISO multi-satellite product? It should be mentioned clearly!

Page 5 line 3: Why does the author stop his study in 2008. In general multi-satellite

OSD

9, C15–C19, 2012

SSH datasets last until at least the end of 2010 and the NCEP reanalysis gives a product which covers the period 1981 to January 2012. This point should be clarified or the period of the study should be extended.

Page 5 line 8: over which period are computed the seasonal cycle of each dataset? Are they computed over the same period?

Page 7, line 24: the assertion "the feature of large oceanic wave is apparent both in the spatial and heterogeneous maps" is not supported by any justification. This has to be documented and justified in details.

Page 9 line 20: what is the dipole index?

In General there are many typing errors and grammar mistakes, far too many to be documented here.

Interactive comment on Ocean Sci. Discuss., 9, 1, 2012.